DEPARTMENT OF
HEALTH, E[illegible]ON AND WELFARE
FEDERAL SECURITY AGENCY
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

Communicable Disease Center
Enteric Bacteriology Laboratories
P. O. Box 185
Chamblee, Georgia

July 1, 1953

Dr. J. Lederberg
Department of Genetics
The University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg:

Under separate cover I am sending a report on your last shipment of cultures. Note that 1966 and 1967, like M66, are S. abortus bovis. 1966 and 1967 are Kauffmann's stock cultures of the two phases of that species and came direct from the stock culture collection. M66 puzzles me but since the number ends in 66, I suspect it should be 1966.

I am indebted to you for the manuscript as edited, to which I have no objections.

As threatened I looked again at SW 1031. I selected only one colony and it went only a --> b and there it is stuck at least temporarily. It went farther for Miss McWhorter. Inasmuch as this is directly related to N97 and the i - b form I suspect the colony selected must play some part. I will repeat this using a number of single colonies.

As to SW 726 and the 1042 series, only SW 1003 and 1042 A2 have proved truly diphasic in my hands. These can be put in serum and plated on successive days, except that SW 1003 spreads rather slowly but this apparently is not due to failure to change phase, it just spreads slowly in the serum medium although distinct swarming is always present after overnight incubation. The remainder of the cultures went a --> e,n,x and there they stuck after serial transfer in n serum.

I agree that it will be hard to reproduce SW 1003. This may be the result of a double hit if one attributes genetic significance to its diphasic behavior. I do not know about this, you have upset my ideas. Why will a bug go e,n,x --> a --> e,n,x and then stick there? Also, the curious behavior of N97 colonies is unexplained. I believe the a - b and i - b forms must parallel N97 in behavior.

I did not make abstracts of the Iseki papers but simply wrote for reprints. Would you return them to me for a few days? I enclose postage. As I remember, Iseki merely grew 4,5,12 forms in 5 serum and obtained 4,12. This does not parallel our experience. Peluffo tried this in 1940 but got no results. We have never had any indication that 5 was subject to

form variation. I am not convinced by Iseki's results. I think you should repeat the work and so shall I if time permits. What reagents will you need?

I note your remarks regarding the 1042 series. None had 5 antigen. Probably the reason you had no more luck with 1042A2 was on account of its very poor motility. This had to be enhanced before satisfactory results were obtained.

It would be very helpful if you would check the Iseki results on group E cultures. I doubt them but whether they are right or wrong, they should be repeated. I shall ask Bruner to send you any transformed group E. cultures which he may have. I am afraid we do not have S. anatum and S. newington from the same animal. Most such occurrences were among the old cultures studied in Kentucky. I shall look into the possibility. I fail to follow your reasoning on this point, except that a transforming phage may be present.

For the Officer-in-Charge, Bacteriology Section

Sincerely yours,

Phil

PRE:mg

Philip R. Edwards, Ph. D.
Bacteriologist-in-Charge
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